Magnesium in Water and Rocks.

The recent publication of analyses of salt in the pans in Cape Colony by Dr. Juritz (*Agricultural Journal*, November, 1908, Cape Town) brings to a head a problem which has been puzzling me for a long time. A large amount of magnesia is dissolved in water on the decay of rocks, yet a very small portion finds its way to the sea. Dead coral reefs become dolomitised, but, as a general rule, recent limestone deposits do not contain more than 1 per cent. of magnesia; the magnesia dissolved in sea-water, therefore, is the accumulation of long ages, and should bear some relation in quantity to that of sodium, yet magnesium in the salts of sea-water is less than one-twelfth that of sodium. In the up-country pans in Cape Colony which collect the water washing over dolerite hills and evaporate the contents on their shallow surfaces, we find plenty of magnesia in the liquors, but practically none in the crystallised product. Here are Dr. Juritz's figures for an average sample:—

Locality	Water, grains per gallon	Salt, per cent.	
Varsch Vley	1204·0	2·13	Lime sulphate
(Ground salt)	Nil	Nil	Lime chloride
	553·0	0·33	Magnesium sulphate
	658·0	1·16	Magnesium chloride
	Nil	Nil	Sodium sulphate
	22050·0	96·43	Sodium chloride
	70·0	Nil	Potassium chloride

Of the seventy-three samples of salt analysed, all tell the same tale; one from Belmont Salt Pan, near Kimberley, contains 7·59 per cent. magnesium sulphate, two contain more than 1·5 per cent., and the rest 1·5 per cent. or under. The ground water, however, struck in wells, is often entirely undrinkable with Epsom salts.

Magnesia compounds, on the other hand, are constantly being drawn down in the earth's crust by the descending surface waters, and cause dolomitisation. The older the limestone, generally speaking, the more it is dolomitised; joints and bedding planes in limestone are dolomitised when the rest is pure limestone, as in the "dunstone" selvages along joints in the Carboniferous Limestone of Durham and Northumberland. Why do the magnesia compounds go downwards and not outwards as the salts of lime and soda do?

The same happens with solutions of iron; practically none reaches the sea, but large amounts descend and replace limestone by spathic iron or hæmatite. In this case one would conclude that the earth's magnetic nucleus exerted a pull on the free iron in solution, which, ceaselessly acting, tended to impoverish the surface of iron. Is there some such action going on with regard to magnesia? Taking Farrington's suggestion that the average composition of meteorites represents the average composition of the earth, then the nucleus should contain a very large proportion of magnesium. Is there any evidence for an attraction of magnesium for magnesium when magnetised as there is in the case of iron for iron?

Ernest H. L. Schwarz.

Rhodes University College, Grahamstown, Cape
of Good Hope, December 21, 1908.

Phosphorescence on a Scottish Loch.

A remarkable illumination was observed about eight years ago on a certain part of Loch Bulig (which lies in the north-western boundary of Aberdeenshire). As it appears to be the only known occurrence of phosphorescence on a Scottish loch, your readers may be interested in it. It appeared in the form of innumerable brilliant lights, shooting rapidly on the surface of the water, but many leaping one or two feet above it. It lasted for about a minute, and was repeated twice at intervals of about ten minutes. The effect was very striking, the brilliance being almost dazzling. It seemed that it could not be accounted for in any other way than by phosphorescent animalculæ, disturbed probably by a shoal of fish which are known to inhabit the loch.

Inquiry elicited the information that near where the lights were seen a soft bank stretched out from the side towards the centre of the loch. I have been desirous since that time to gather some of the deposit, if possible, for examination, but only a few months ago was I able to carry out my intention. I found it was a matter of no little difficulty, as the loch at that part is about 25 feet deep, and though it is usually quite smooth it sometimes is somewhat rough. The first attempt was a failure, the day being squally, the waves 2 or 3 feet high, and the strong wind and current rendered it difficult to locate the bank and collect specimens. The second attempt, however, was successful, and I found that the bottom was generally stony, but gave place to soft material just above where the lights had been seen. I collected two quantities of the deposit, and found that it consisted of sand mixed with a large quantity of carbonaceous matter, mostly in the form of small rolls, half an inch to one inch long. Microscopic examination showed that these rolls contained animals encased like tubicolous annelids; they were quite active, emerging from the tube, grasping black particles, and then retreating; some were encased in parchment-like tubes, through which the rapid actions of the animal could be distinctly seen; one was found with a transparent tunic, hanging by a ring from the neck, resembling *Oxyethira costalis* (Hydrophilideæ); I still have this specimen. Along with these and other animals were numerous diatoms, nematodes, &c. As some of these animals belong to classes which are known to be phosphorescent, it seems that their presence in the deposit is sufficient to account for the remarkable appearance seen. This was confirmed by finding that the sand contained much more phosphate than sand usually contains; also, by testing with ammonium molybdate some of the black matter, including one of the black rolls containing an animal, after a few hours a distinct yellow precipitate was found, but only in the vicinity of the black roll.

I should think that this deposit would form an interesting preserve for zoologists, and therefore I relate the circumstance, and shall be glad to give any further information to anyone who may desire it.

Thos. Jamieson.

Chemical Laboratory, 10 Belmont Street, Aberdeen.

[It is to be hoped that Mr. Jamieson will re-observe the interesting phenomenon he saw on Loch Bulig and collect material *at the time*. If he saw numerous luminescent organisms leaping in the air, they may *possibly* have been Chironomids with phosphorescent bacteria. He gives no convincing evidence in his letter that the organisms collected from the deposit were connected with the "phosphorescent" display. We may recall the fact that a "phosphorescent" Enchytræid has been reported in Britain.—Ed. Nature.]

The Movement of Water in Soils.

In Nature of August 6, 1908, Dr. Russell refers, in his note on *soil moisture*, to some information on this subject which I published in Memoir No. 6 (Chemical Series) of the Department of Agriculture in India, and says, "Dr. Leather argues that water moves upwards from a limited depth only. . . . The results are equally well explained on the supposition that the upward movement takes place at all depths, since the amount of water present in a particular layer depends on the respective rates at which water is gained from below and lost to the upper layers."

I still maintain that the process of upward movement of water through a soil during dry weather is one which is not brought into operation throughout all strata of a soil instantaneously, but that, on the contrary, time is required for it to be communicated through succeeding strata; consequently, during a dry period there will be strata in which the process has not yet become established. Until an alteration of the surface tension occurs in any stratum there can be no movement (due to this cause) of water, and a *necessary* result of the fact that this alteration of surface tension through succeeding strata is *gradual* is that any such alteration *must* be accompanied by a decrease of water per cubic foot.

A second consequence is that if the total decrease of water which occurs during a dry period throughout the

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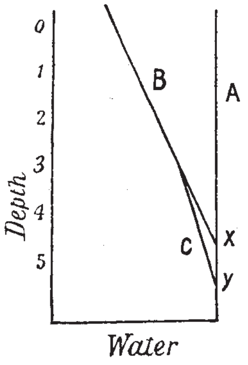
strata is ascertained, this will be precisely that quantity of water which has evaporated from the surface of the land.

The only weakness that I have been able to perceive in the conclusions which I drew in 1907 regarding this matter is the exact depth which was affected. This I concluded to be 7 feet in the Pusa soil. As a matter of fact, the ascertainment of this depth with exactitude is not possible. In the marginally shown diagram are three curves. A represents the water immediately after rain ceases (in an ideal physically uniform soil). B represents the water as ascertained at the conclusion of the dry period, and cuts A at the point x, below which no decrease of water has been perceptible by the methods employed. Owing, however, to errors, due principally to difference of physical character of soil, this point cannot be determined exactly, and in reality the curve might be BC, that is, it might cut A at, say, y, and not at x; but the difference between the ascertained loss and the real loss can only be trifling. It is perhaps needless to add that this error does not affect the principles involved. It is naturally assumed that the points x and y are above the stratum of soil which is maintained in a saturated condition by the underground water.



J. WALTER LEATHER.

Pusa, Bengal, November 23, 1908.

DR. LEATHER is no doubt correct in supposing that the upward movement of water through the soil is gradual, and in his further deduction that there must, for a time, be some strata in which the water has not yet begun to move; but we do not know the velocity at which water travels upwards in the soil, and consequently one cannot say whether the time during which any particular stratum remains unaffected is to be measured in days or months. Dr. Leather's results do not give the velocity of upward movement, but the difference between the loss and the gain of water at different depths. It is no more possible to calculate the amount of water that has passed through a particular stratum by determining the amounts present at two different times than it would be to calculate the quantity of heat passing along a rod of unknown thermal properties by measuring the temperature change at a particular point.

The great value of Dr. Leather's results lies in the fact that they are the most complete set of moisture determinations yet made under conditions of drought. If only some physicist could be induced to turn his attention to soil problems he would find these *data* very useful.

E. J. RUSSELL.

The Rothamsted Laboratories, Harpenden, Herts.

The Correlation of Teaching.

THE valuable summary of Prof. Perry's address to the "correlation" conference given in NATURE of December 3, 1908 (p. 143), contains the following statement:—"If a boy wrote a description of anything he had done in a laboratory or elsewhere, it should be an exercise in English." This is, *unfortunately*, accepted by educationists at the present time. Can Prof. Perry not aid in breaking down this barrier to progress rather than in fixing its joints more firmly? He has done so much, cannot he do more?

I would suggest, whether in class-room exercises or in examinations, that the boy's resultant essay should be examined and corrected by the examiner without reference to spelling, writing, grammar, &c. The object should surely be to put on paper what has been learnt about the subject in hand. Spelling being the result of the use of accurate vision, writing being the result of physical and nervous stability, need in no circumstances affect the value of a scientific production. As the boy gets older and as his view of life extends, the value of his production, from the point of view of English, will gradually improve.

Besides, it is nothing short of an absurdity to look at certain exercise books, where the red-ink corrections, that have taken so much of the teacher's time, are chiefly connected with something that in no way affects the value of the exercise itself.

CHARLIE WOODS.

December 31, 1908.

I AGREE with Mr. Woods in his condemnation of a system in which every exercise is treated as if grammar and composition were as important as the subject-matter; but surely he is going too far when he says that in writing a description of what he has done or seen, a boy need not fear that his grammar or spelling or composition will be criticised at all. I ask the science master, in teaching science, to teach also mathematics and English and sketching incidentally, and to take some pleasure in doing it. If he insists that these subjects are the absolute preserves of the mathematical, the English, and the drawing masters, he must not be astonished when a classical master openly expresses pride in an ignorance of "stinks."

I do not think that the system which Mr. Woods condemns is very much in vogue, whereas the system of which he approves has done an immense amount of harm. We all of us know men holding the highest science degrees whose spelling, grammar, and composition are beneath the contempt of a board-school boy. In nine cases out of ten it will be found that when a student cannot give a clear account of what he has seen or done, he has no clear ideas about the matter, and every examiner knows that it is only the very exceptional man who has clear knowledge yet cannot express himself clearly. If the examiner has a keen sense of justice this candidate gives him more trouble than any five others.

JOHN PERRY.

An Electromagnetic Problem.

THE problem noticed by Mr. Comstock in NATURE of November 19, 1908, is an interesting one, but I do not see how the "laws of electricity and conservation of energy require in themselves the discrete structure of electricity or the association of electricity with matter." The electromagnetic field produced by a uniform spherical sheet of electricity, unassociated with matter expanding under its own repulsion, is not zero, but indeterminate. The total energy of the system remains finite and constant, while the velocity of expansion is that of light. Thus perfect uniformity of electricity, together with isolation, is not incompatible with the laws of electricity and conservation of energy. The indeterminateness of the electromagnetic field will, of course, surprise no one who is willing to start with a distribution of electricity differing infinitely little from that of perfect uniformity, arranged as a sheet differing infinitely little from spherical, and expanding in surroundings departing infinitely little from the symmetrical.

A. CORE.

MR. CORE'S objection would apply in many problems where certain functions appear to vanish because of symmetry, but in the present case I think it does not apply.

In physical problems it is well to avoid mathematical "sheets" except in unusually simple circumstances, and in the present case the spherical shell of electricity which is expanding under the mutual repulsion of its parts is to be considered of *finite thickness* and of constant volume density of electricity.

In these circumstances the displacement current is evidently equal and opposite to the convection current when the sphere is expanding, and hence the curl of the magnetic force is zero everywhere. This requires the magnetic force to be zero everywhere, since such a vector vanishing at infinity and having its curl and divergence both zero must itself vanish. It is not then immediately evident what becomes of the electrical energy lost on expansion.

D. F. COMSTOCK.

Institute of Technology, Boston, December 17, 1908.

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